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A comparison of transplant outcomes in peritoneal and hemodialysis patients

To the Editor: The recent study published in *Kidney International* by Snyder et al [1] examines the impact of dialysis modality at the time of renal transplant on the outcome of the transplant intervention. Confirming previous reports [2], early graft function rates are significantly better for peritoneal dialysis patients than for hemodialysis patients. Also, early graft function is disclosed as a significant predictor of graft survival. Somewhat paradoxically, early graft survival appears to be significantly worse in peritoneal dialysis patients, a difference attributed to higher rates of graft thrombosis in this group.

The size of the sample and the amount of information presented by the authors are really impressive, but the main conclusion of the study is weakly supported by the presented data. The difference in graft survival between peritoneal dialysis and hemodialysis is small, well below the limits that many would consider clinically significant. In addition, this difference is statistically significant only in the first 3 months after transplantation, which sheds further doubts on the real relevance of the results. Finally, statistical differences are clearly significant only if early demises are censored. Death in the first weeks after transplantation may be linked to graft dysfunction and, in our opinion, death must be considered an instance of graft failure in this setting, to yield a more realistic view of the question.

Even if we accept the main conclusions of the study, Snyder et al [1] do not provide an answer for the main question. Are their findings attributable to features specific to the mode of dialysis, or just the consequence of a disequilibrium in the distribution of other risk factors for graft thrombosis between both populations? The study evaluates a wide set of covariables, but we miss data that are essential to analyze this question (e.g., immunosuppression schedules, use of right versus left kidney, use of very-low age donors or, importantly, analysis of potential center effects or differences in the prevalence of thrombophilic states between both populations). Unless these points are clarified, it cannot be discarded that the mode of dialysis may be simply a confusion variable.

We agree with the authors that further studies are necessary to settle these questions.

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Reply from the Authors

We disagree that our finding, that patients treated with peritoneal dialysis versus hemodialysis have slightly worse graft survival after kidney transplantation, is weakly supported by the data [1]. While the difference in the rate of graft failure between these two groups is small, this finding, nevertheless, contradicts the hypothesis that early graft survival among peritoneal dialysis patients should be better. The fact that the reduction in graft survival among peritoneal dialysis patients was statistically significant during the first 3 months, but not among patients who survived at least 3 months, does not diminish the importance of the finding.

We disagree that differences are clearly significant only if deaths are censored. The effect of dialysis modality on outcome was seen for both death-censored and overall graft failure. There was no effect on mortality, implying that most of the effect of dialysis modality on graft failure was due to its effect on death-censored graft failure.

We cannot tell from these data why peritoneal dialysis patients have reduced graft survival, despite having less delayed graft function. We can think of no plausible reasons why there should be differences between the two groups with respect to whether the right or left kidney was used (left was used 55% in both groups), or in the proportion of very young donors (donors <2 or <5 were used in 1% and 2%, respectively, in both groups). While there were minor differences in the type of immunosuppression used in the two groups, adjusting for this did not change the effect of peritoneal dialysis versus hemodialysis on graft survival.

We can only speculate whether peritoneal dialysis patients may have had a higher prevalence of inherited coagulopathies. Indeed, our observation that the higher rate of graft failure may have been due to a higher rate of graft thrombosis must be tempered by the fact that only a subset of patients had data on the cause of graft failure and could be included in this analysis. Clearly,

additional studies are needed to better understand these results.

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Overnight unattended home hemodialysis

To the Editor: Precedent claims are rarely correct, thus, the statement by Dr. McFarlane, Dr. Pierratos, and Dr. Redelmeier in a recent issue of *Kidney International* that they invented unattended self home overnight hemodialysis in Toronto in 1993 [1] cannot go unchallenged. I believe that my group and I were the first to develop this technique 30 years earlier and that we published extensively in the period 1963 to 1968 on this subject [2–6]. In addition, visual proof of my claim can be seen in a video of self-unattended overnight home hemodialysis in 1968 by any of your readers at *www.mybesthealth.com/shaldon/wmv/cahmed.wmv* [7].

However, the authors are to be congratulated on their excellent emphasis on the cost effectiveness of this type of dialysis, which was the main goal of our development of the technique in a cash-starved National Health Service in the United Kingdom.

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Reply from the Authors

We would like to thank Dr. Shaldon for his letter and his complimentary words on our work. Dr. Shaldon's pioneering work in the area of home dialysis is well known, undisputed, and remarkable, especially taking into account the technical limitations of the time [1]. We never disputed the fact that home hemodialysis has been done before at night, or that both long dialysis [2], as well as daily dialysis [3] were well established regimens. Our contribution is the successful combination of these three elements. To our knowledge from the review of the literature, Dr. Shaldon's home nightly dialysis was performed three and rarely four times a week. We believe that the combination of all three elements—high frequency, long duration, and the location at home is responsible for the success of our regimen, as has recently been pointed out by Kooistra [4].

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Influence of dialysis membranes on outcomes in acute renal failure

To the Editor: In their recent meta-analysis published in *Kidney International,* Subramanian, Venkataraman, and Kellum [1] used a fixed effects model to combine the results of the studies addressing the impact of synthetic dialysis membranes on survival in acute renal failure. In addition, their primary analysis pooled results from an observational study with those from randomized/quasi-randomized trials. In our opinion both of these approaches are inappropriate.

Fixed effects models assume that the effect of treatment